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Strangers in Hostile Lands: Exposure to Refugees and Right-Wing Support in Germany's Eastern Regions

Comparative Political Studies

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Supporting Information for ‘Strangers in Hostile Lands’

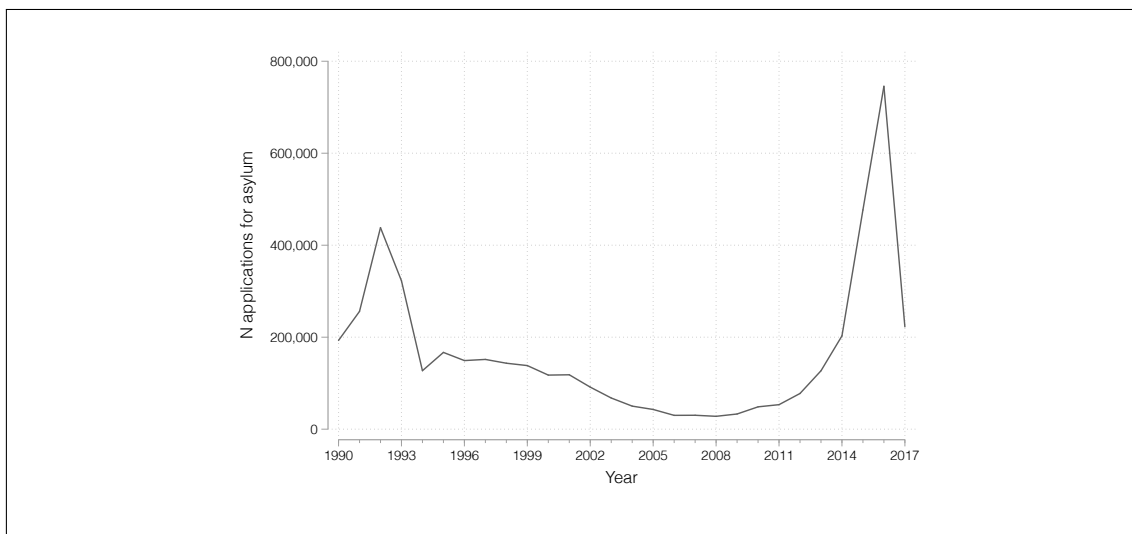
Table of Contents

A	Further information on asylum applications and shares of foreigners in Germany	1
B	Measurement of mechanisms	2
B.1	Heterogeneous treatment effects on mechanisms	4
C	Allocation of refugees	5
C.1	Allocation of refugees at the county level	5
C.2	Difference in available housing due to diverging economic trends	6
D	Regression tables and additional results	7
D.1	Heterogeneous treatment effects	13
D.2	Regression using age and gender weights	14
E	Robustness checks	14
E.1	Treatment dosage	14
E.2	Perceived vs. actual treatment	16
E.3	Spatial spillover	17
E.4	‘Prevented’ refugee allocation due to violence against refugees or refugee housing	19
E.5	Attrition	20
E.6	Origin effects	21
F	Behavioral games	23
G	Simulation of selection on unobservables	25
H	Sampling and recruitment procedures	26
H.1	Recruitment dynamics over time	30
	References	32

A Further information on asylum applications and shares of foreigners in Germany

Figure 9A shows the number of asylum applications in Germany for the years 1990 to 2017. A clear peak is visible for the years 2014–2016, the years of the ‘refugee crisis’. Even though most refugees arrived in Germany in 2015, the highest number of asylum applications was registered in 2016. This is because the mass arrival of refugees in 2015 put the German asylum system under strain. Many applications could therefore only be processed in the following year.

Figure 9A: Number of applications for asylum in Germany since 1990



Data provided by the German Federal Agency for Civic Education (bpb, 2018).

Table 4A: Share foreign citizens in population in Eastern and Western Germany/Europe

	Germany		Europe	
	West	East	West	East
Share foreign citizens	10.5%	2.7%	8.6%	2.0%
<i>Western and Southern Europe:</i> Belgium, Denmark, Germany, Ireland, Greece, Spain, France, Italy, Cyprus, Luxembourg, Malta, Netherlands, Austria, Portugal, Finland, Sweden, United Kingdom, Iceland, Liechtenstein, Norway, Switzerland; <i>Central and Eastern Europe:</i> Bulgaria, Czech Republic, Estonia, Croatia, Latvia, Lithuania, Hungary, Poland, Romania, Slovenia, Slovakia. Calculated from Eurostat (2015).				

Table 4A compares the share of foreigners in Western and Eastern Germany to those in Western and Southern vs. Central and Eastern Europe. As can be seen, the share of foreigners Western Germany is most similar to that for Western and Southern Europe, while the figure for Eastern Germany resembles that for Central and Eastern Europe.

B Measurement of mechanisms

Indicators for the six explanatory mechanisms were built using several survey items. Economic competition and relative deprivation are captured by a combination of objective and subjective measures recording whether a person is unemployed, has a household income that is in the lowest quartile of the observed income distribution, and feels that s/he does not receive her fair share relative to others in Germany. Personal security is measured with items on proximity to areas deemed unsafe in the dark, and the perceived likelihood of becoming a victim of theft or physical violence. Cultural alienation, a more abstract concept, requires several survey items, including whether ‘due to the many foreigners in Germany, one increasingly feels like a stranger in one’s own country.’ We probe for dissatisfaction with the elites with the statement ‘The authorities are doing too much for refugees, and too little for German citizens,’ and for community-discord with an item on whether the issue of refugees has polarized their community. Empathy is measured by inquiring to what extent respondents pity and commiserate with refugees (Oceja et al., 2014). Finally, contact is captured with questions asking whether individuals a) have encountered refugees on the street, at work, or at their children’s school and b) whether individuals have been actively involved in supporting refugees. As before, for parsimony of presentation, we combine the measures for each mechanism in a scale by standardizing and averaging over items. Table 5A includes the full list of measures and their values across treatment conditions.

Table 5A: Summary statistics mechanisms

	Overall		Control		Treatment	
	Mean	SD	Mean	SD	Mean	SD
<i>Deprivation</i>						
Lowest income quartile	0.25	(0.43)	0.26	(0.44)	0.24	(0.43)
Unemployed	0.03	(0.18)	0.04	(0.19)	0.03	(0.17)
Bad personal economic situation	2.16	(0.70)	2.20	(0.72)	2.14	(0.68)
Receive less than fair share	3.52	(0.80)	3.51	(0.84)	3.53	(0.77)
Eastern Germans treated as second class	2.27	(0.86)	2.32	(0.89)	2.23	(0.84)
Fear to end up on losing side	2.26	(0.95)	2.29	(1.00)	2.23	(0.92)
<i>Insecurity</i>						
Fear entering neighborhood alone	0.15	(0.36)	0.14	(0.35)	0.16	(0.36)
Fear theft	2.46	(0.93)	2.48	(0.99)	2.45	(0.89)
Fear physical violence	2.01	(0.67)	1.98	(0.68)	2.03	(0.66)
<i>Cultural alienation</i>						
Refugees culturally enrich society	2.41	(0.89)	2.44	(0.91)	2.39	(0.87)
Feel like stranger in own country	2.17	(0.95)	2.25	(0.99)	2.12	(0.92)
Islam trying to extent reach	2.84	(0.93)	2.84	(0.96)	2.84	(0.92)
Muslims reject German constitution	2.55	(0.83)	2.51	(0.85)	2.57	(0.81)
<i>Discord</i>						
Authorities favor refugees	2.62	(0.99)	2.66	(1.02)	2.59	(0.98)
Refugee topic divided community	0.17	(0.37)	0.18	(0.38)	0.16	(0.37)
Opinion differ in politics discussions	2.96	(0.74)	2.99	(0.79)	2.94	(0.71)
Not understand anti-refugee demonstrations	2.46	(0.87)	2.43	(0.89)	2.49	(0.85)
Not understand helping refugees	1.52	(0.67)	1.57	(0.70)	1.49	(0.64)
<i>Empathy</i>						
Feel pity with refugees	2.73	(0.79)	2.74	(0.80)	2.73	(0.78)
<i>Contact</i>						
Contact with refugees on the street/school	0.43	(0.50)	0.41	(0.49)	0.45	(0.50)
Provided personal support to refugees [†]	0.15	(0.35)	0.11	(0.31)	0.17	(0.37)
Foreign acquaintances	1.95	(1.04)	1.93	(0.99)	1.97	(1.06)
N	1,320		512		808	

Differences between treatment and control condition significant at $p < 0.05$ marked with a dagger (†) symbol. The only mechanism significantly ($p < 0.05$) affected by the treatment is intense contact ('Provided personal support to refugees'). This finding does not extend to more loose forms of self-reported contact, like seeing refugees on the street.

B.1 Heterogeneous treatment effects on mechanisms

Figure 10A: Heterogeneous treatment effects on mechanisms for those aged over 40 and under

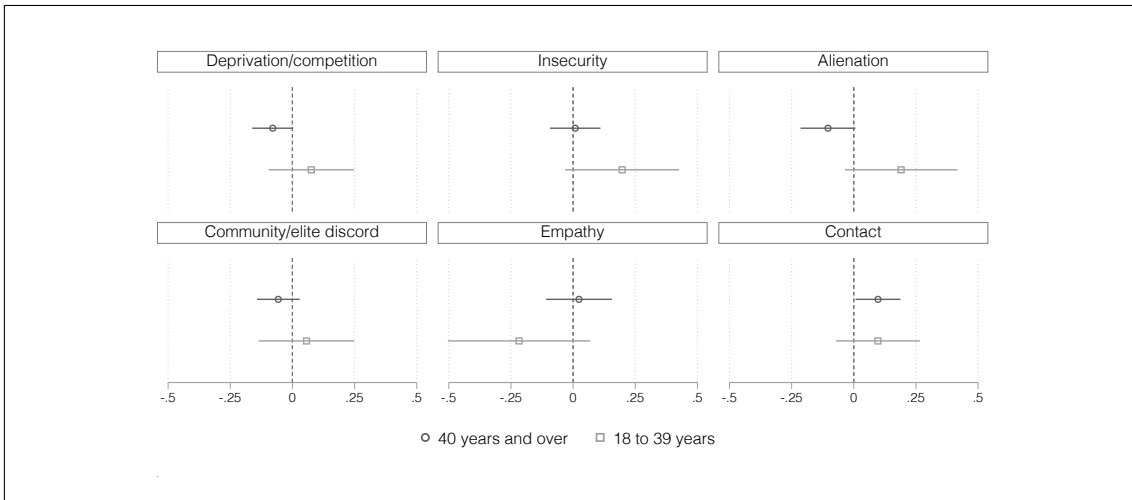
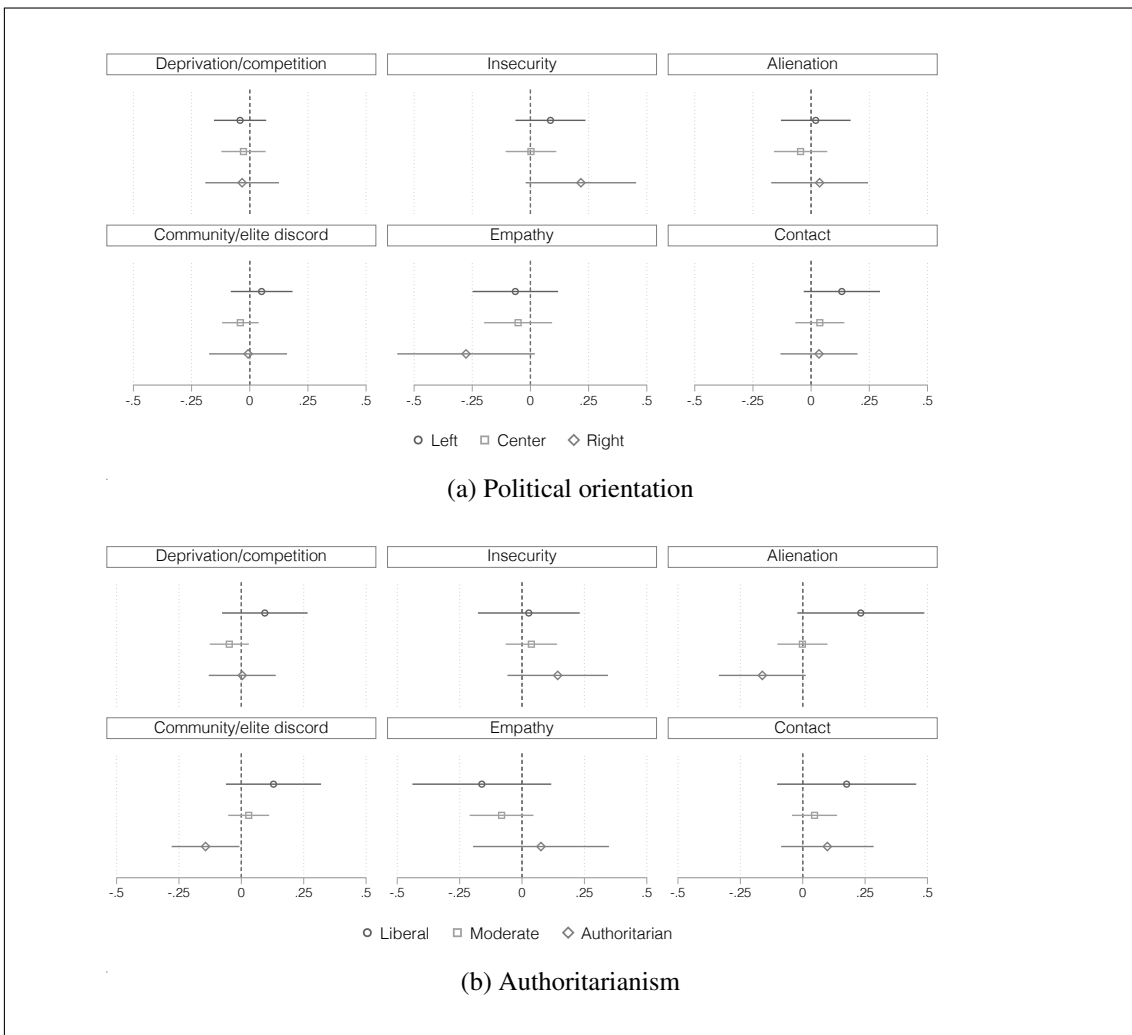


Figure 11A: Heterogeneous treatment effects on mechanisms for political orientation and authoritarianism



C Allocation of refugees

C.1 Allocation of refugees at the county level

In Table 6A we check whether the allocation of refugees was independent of county characteristics other than population size, as stipulated by the allocation policy. We find this idea largely confirmed. The table shows a regression of the number of refugees per 100 residents (i.e. proportional to population size) allocated to a given county in 2015 on pre-treatment covariates. The unit of analysis are the 76 counties (*Landkreise*) in the five Eastern German states (excluding Berlin). Given the other predictors, the allocation of refugees is independent of the political leaning of a county, the county's purchasing power and unemployment rate, average age and demographic composition, and does not differ systematically between states (*Bundesländer*). The only predictor approaching conventional levels of significance ($p=0.051$) is population density, with more densely populated counties being allocated somewhat higher numbers of refugees.

Table 6A: Regression of refugee allocation on county characteristics

	Municipality hosting refugees		
	b	se	ci95
Population 2014	-0.00	0.00	-0.00,0.00
Population density 2014	0.00	0.00	-0.00,0.00
Unemployment rate 2014	-3.34	9.27	-21.9,15.2
Share women in 2011	-0.30	0.18	-0.66,0.06
Average age in 2011	-0.07	0.25	-0.57,0.42
Share of population over 65 in 2011	0.05	0.12	-0.19,0.28
Share of men 15 to 25 years in 2011	2.78	19.81	-36.9,42.7
Share foreigners	-0.03	0.18	-0.39,0.33
Purchasing power pp 2005	-0.00	0.00	-0.00,0.00
Voter turnout in %	-0.03	0.03	-0.09,0.02
Share CDU votes in %	0.03	0.08	-0.18,0.13
Share SPD votes in %	-0.03	0.01	-0.22,0.16
Share LINKE votes in %	-0.01	0.08	-0.17,0.15
Share OTHERS votes in %	-0.05	0.09	-0.23,0.14
Brandenburg (ref.)			
Mecklenburg-Vorpommern	-0.37	0.14	-1.12,0.45
Sachsen	-0.27	0.51	-1.30,0.75
Sachsen-Anhalt	-0.60	0.48	-1.56,0.37
Thüringen	-0.53	0.42	-1.38,0.31
Share empty housing	0.10	0.06	-0.01,0.21
Constant	23.60	14.93	-6.30,53.51
<i>N</i>	76		
<i>F</i> (19, 56)	1.38		
<i>R</i> ²	0.088		

Regression of the number of refugees per 100 inhabitants per county on pre-treatment covariates. Coefficients significant at the 5%-level marked with a dagger symbol (†).

C.2 Difference in available housing due to diverging economic trends

As pointed out by one of our reviewers, we might suspect that refugees were more often sent to places where the housing market was weak. While our treatment and control variables were closely matched on economic variables, notably purchasing power per capita, dynamics in the run-up of the crisis could have differed. If the economic situation in the treatment municipalities was on a downward trend, housing here might have been cheaper, which would explain why refugees were sent here. This would likely tend to bias findings towards finding a negative effect, since the negative economic outlook may have reduced the openness towards the newcomers. Our data allows us to tentatively test this idea. For several variables, we have data for both 2005 and 2015. We can thus compare trends leading up to the refugee crisis.

Figure 12A: Pre-treatment trends in population numbers, housing and purchasing power

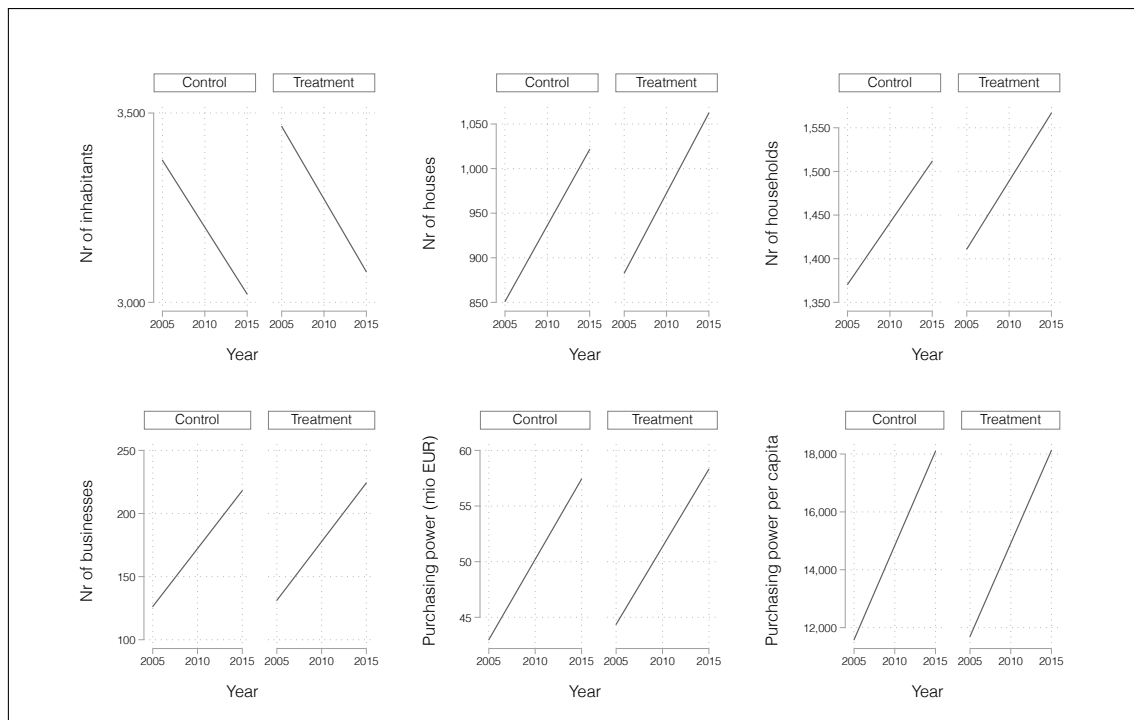


Figure 12A shows that pre-treatment trends followed a very similar pattern, even though we did not match on trends. In no cases are the differences-in-difference between the years 2005 and 2015 and the treatment and control municipalities close to statistically significant (all interactions $p > 0.5$). This analysis therefore increases our confidence that the municipalities we are comparing were structurally extremely similar.

D Regression tables and additional results

Table 7A presents the full results for Figure 5 in the main text. Regression results with treatment effects for the individual-level indicators shown in columns 1 to 4. Among the individual-level covariates, only being female and the level of education have strong and precisely estimated coefficients. Women score between 0.09 and 0.16 standard deviations lower on the behavioral bias-, right-wing- and refugee rejection-scales than men. Higher education is strongly associated with lower bias, right-wing-support, populist attitudes and rejection of refugees. Looking at the municipality-level covariates, right-wing support is predicted by the AfD vote share in 2013, and the population in some states appears to systematically support populist positions more strongly than that in others. Other effects are zero or close to zero. In general, the included covariates perform relatively poorly in predicting our outcomes of interest, with r-square values between 0.04 and 0.09. Column 5 presents the results of a municipality-level regression of the AfD vote share in the 2017 general elections on the treatment indicator and covariates. Here, the election results from the previous election show themselves to be strong predictors, as do turnout and the share of males between 15 and 25 years. The regression does a good job at explaining variation in the outcome, with an r-square of 0.69.

However, absent from the list of significant predictors is our treatment in focus. Whether a municipality has accepted refugees or not hardly matters, with treatment municipalities only showing 0.29 percentage points higher support for the AfD. Figure 14A shows results for regressions of all individual outcome measures on the treatment indicator, simultaneously controlling for the full set of individual- and municipality-level pre-treatment covariates.

Figure 13A puts our treatment effects in perspective by plotting them alongside individual-level predictors. The Figure demonstrates that our null effects are precisely estimated (e.g. with similar precision as the predictive effect of gender), and are closer to zero than virtually all of the other predictors. We also see that some variables do predict our outcomes quite strongly, notably gender and education.

Figure 15A shows plots for regressions of the individual indicators on the treatment. Finally, Figure 16A replicates the coefficient plot in Figure 5 in the main text, weighting observations to match the age and gender margins of the population in our sample municipalities.

Table 7A: Regression of outcome measures on treatment status

	(1)		(2)		(3)		(4)		(5)	
	AfD 2017		Right-wing support		Anti-refugee sentiments		Populism		Behavioral bias	
Treatment municipalities	0.29	(0.51)	-0.02	(0.04)	-0.02	(0.04)	-0.03	(0.04)	-0.02	(0.05)
Female			-0.12**	(0.04)	-0.09*	(0.04)	0.05	(0.04)	-0.13**	(0.05)
Age			-0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)
Single (ref.)										
Stable union			0.12	(0.07)	0.17*	(0.07)	0.11	(0.07)	0.01	(0.08)
Separated			0.16	(0.09)	0.16	(0.10)	0.07	(0.09)	0.00	(0.11)
Widowed			0.10	(0.10)	0.15	(0.10)	0.20*	(0.10)	-0.03	(0.13)
Children			-0.01	(0.02)	-0.06*	(0.03)	-0.04	(0.02)	0.00	(0.04)
Household size			0.00	(0.02)	0.01	(0.02)	-0.02	(0.02)	-0.00	(0.03)
Education			-0.14***	(0.02)	-0.14***	(0.02)	-0.09***	(0.02)	-0.09***	(0.02)
Area in km2	-0.01	(0.01)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)
Population	0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)
Population density	-0.01	(0.01)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)
Unemployment	0.31	(0.21)	0.01	(0.02)	-0.01	(0.02)	0.03	(0.02)	0.00	(0.02)
Share female	0.02	(0.21)	-0.01	(0.02)	-0.01	(0.02)	-0.01	(0.02)	-0.02	(0.02)
Average age	-0.15	(0.27)	0.03	(0.03)	0.06*	(0.03)	-0.00	(0.03)	-0.01	(0.03)
Share over 65 years	0.06	(0.13)	-0.01	(0.02)	-0.02	(0.01)	0.01	(0.01)	0.00	(0.01)
Share men 15-25 years	1.03**	(0.39)	-0.05	(0.04)	-0.04	(0.04)	-0.04	(0.04)	-0.09	(0.07)
Share foreigners	-0.68	(0.93)	-0.09	(0.08)	0.02	(0.10)	0.15	(0.09)	-0.16	(0.10)
Purchasing power	-0.25	(0.17)	-0.01	(0.01)	-0.01	(0.02)	0.01	(0.02)	0.02	(0.02)
Turnout 2013	-0.18***	(0.05)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.01	(0.01)
Vote share CDU 2013	-0.36***	(0.10)	0.02*	(0.01)	0.01	(0.01)	-0.00	(0.01)	0.01	(0.01)
Vote share SPD 2013	-0.67***	(0.12)	0.01	(0.01)	-0.00	(0.01)	0.01	(0.01)	0.02	(0.01)
Vote share Linke 2013	-0.17	(0.12)	0.02*	(0.01)	0.02	(0.01)	0.01	(0.01)	0.02	(0.01)
Vote share AfD 2013	0.84***	(0.19)	0.05**	(0.02)	0.02	(0.02)	-0.00	(0.03)	0.02	(0.02)
Brandenburg (ref.)										
Mecklenburg-Vorpommern	-4.88***	(1.07)	-0.07	(0.09)	-0.10	(0.11)	0.25*	(0.11)	-0.07	(0.12)
Sachsen	7.80***	(1.26)	0.01	(0.09)	-0.10	(0.10)	0.30***	(0.09)	-0.01	(0.13)
Sachsen-Anhalt	-2.41*	(1.20)	0.07	(0.10)	-0.14	(0.12)	0.15	(0.11)	0.01	(0.12)
Thüringen	0.35	(1.21)	0.06	(0.09)	-0.08	(0.10)	0.23*	(0.09)	-0.05	(0.10)
Constant	65.00***	(16.28)	-1.67	(1.44)	-2.25	(1.53)	-0.13	(1.73)	0.20	(1.89)
Observations	236		1,319		1,311		1,314		1,320	
R ²	0.69		0.09		0.08		0.05		0.03	

OLS regression. Difference to full sample size (n=1,320) due to missing items on individual outcome measures. SE clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 13A: Coefficient plot comparing treatment effect to predictive effect of covariates

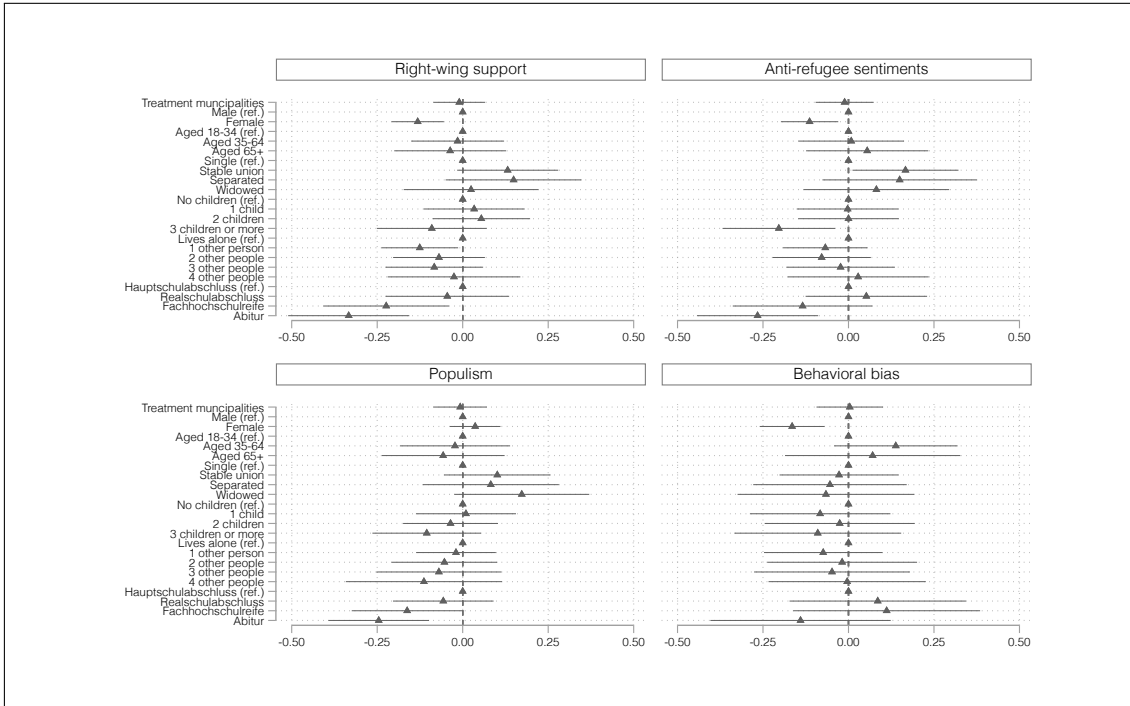


Table 8A: Regression of outcomes on theoretical mechanisms

	(1) Right-wing support		(2) Anti-refugee sentiments		(3) Populism		(4) Behavioral bias	
Deprivation/competition	0.01	(0.02)	-0.02	(0.03)	0.23***	(0.03)	-0.10*	(0.05)
Insecurity	0.02	(0.02)	0.04	(0.02)	0.08**	(0.03)	0.01	(0.04)
Alienation	0.39***	(0.02)	0.33***	(0.03)	0.18***	(0.03)	0.27***	(0.05)
Community/elite discord	0.28***	(0.03)	0.25***	(0.03)	0.19***	(0.04)	0.11	(0.06)
Empathy	-0.07***	(0.02)	-0.15***	(0.02)	-0.00	(0.02)	-0.08*	(0.03)
Contact	0.01	(0.02)	-0.06**	(0.02)	-0.02	(0.03)	-0.02	(0.04)
Constant	-1.10	(0.97)	-1.76	(1.07)	0.86	(1.59)	1.05	(1.85)
Individual controls	Yes		Yes		Yes		Yes	
Municipality controls	Yes		Yes		Yes		Yes	
Observations	1,175		1,174		1,175		1,175	
R ²	0.68		0.64		0.31		0.17	

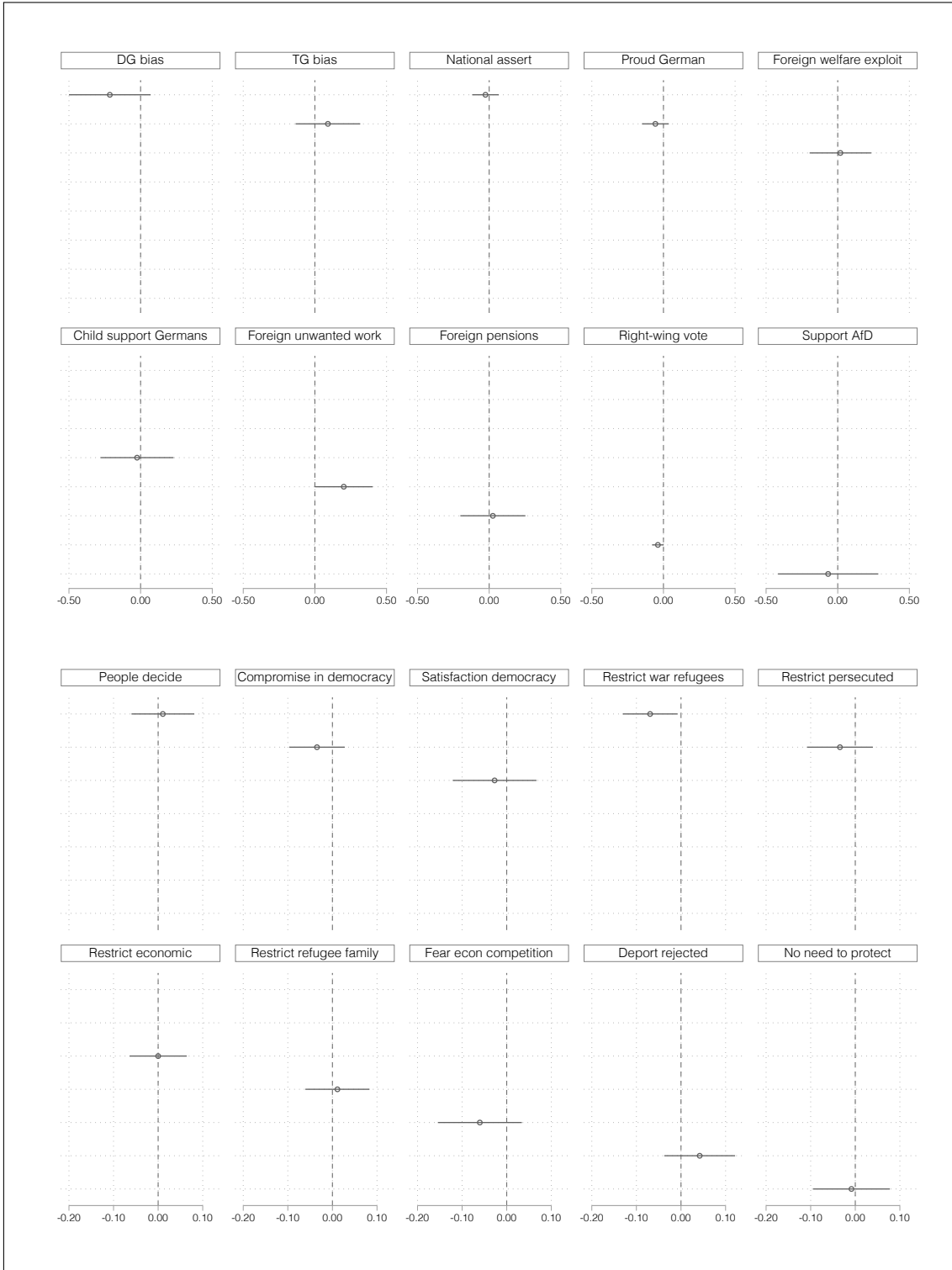
Cluster robust standard errors in parentheses. SE clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9A: Regression of mechanisms on treatment status

	(1)		(2)		(3)		(4)		(5)		(6)	
	Deprivation/competition		Insecurity		Alienation		Community/elite discord		Empathy		Contact	
Treatment municipalities	-0.02	(0.03)	0.04	(0.04)	-0.03	(0.05)	-0.02	(0.03)	-0.05	(0.05)	0.08*	(0.04)
Female	0.05	(0.04)	0.02	(0.05)	-0.04	(0.05)	0.01	(0.04)	0.25***	(0.06)	-0.04	(0.04)
Age	0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.01**	(0.00)	-0.01***	(0.00)
Single (ref.)												
Stable union	-0.19**	(0.06)	-0.05	(0.10)	0.09	(0.08)	0.10	(0.07)	-0.03	(0.11)	0.08	(0.07)
Separated	0.03	(0.11)	-0.08	(0.12)	0.14	(0.12)	0.15	(0.10)	0.03	(0.15)	0.15	(0.10)
Widowed	-0.12	(0.10)	-0.00	(0.14)	0.15	(0.12)	0.13	(0.10)	0.09	(0.18)	-0.05	(0.11)
Children	-0.04	(0.02)	-0.03	(0.03)	-0.06	(0.03)	-0.06**	(0.02)	0.03	(0.04)	0.04	(0.03)
Household size	0.09***	(0.02)	0.00	(0.03)	0.02	(0.03)	0.04	(0.02)	0.01	(0.03)	-0.01	(0.02)
Education	-0.16***	(0.02)	-0.07**	(0.03)	-0.16***	(0.02)	-0.15***	(0.02)	0.17***	(0.03)	0.14***	(0.02)
Area in km2	-0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Population	-0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)
Population density	-0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Unemployment	0.02	(0.02)	0.00	(0.02)	0.00	(0.02)	-0.00	(0.02)	-0.01	(0.03)	0.00	(0.02)
Share female	0.02	(0.02)	-0.03	(0.02)	-0.03	(0.02)	-0.02	(0.02)	0.01	(0.03)	-0.01	(0.02)
Average age	0.05*	(0.02)	0.03	(0.03)	0.02	(0.03)	0.00	(0.02)	-0.02	(0.04)	-0.04	(0.03)
Share over 65 years	-0.01	(0.01)	-0.01	(0.01)	0.01	(0.01)	0.00	(0.01)	0.01	(0.02)	0.02	(0.01)
Share men 15-25 years	0.03	(0.04)	-0.00	(0.06)	-0.06	(0.05)	-0.05	(0.04)	0.04	(0.06)	-0.05	(0.04)
Share foreigners	0.11	(0.07)	-0.11	(0.09)	-0.02	(0.11)	0.02	(0.09)	-0.17	(0.12)	0.06	(0.09)
Purchasing power	-0.03	(0.02)	0.01	(0.02)	-0.02	(0.02)	-0.01	(0.01)	0.03	(0.02)	-0.00	(0.02)
Turnout 2013	-0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	-0.01	(0.01)	0.00	(0.00)
Vote share CDU 2013	-0.00	(0.01)	-0.00	(0.01)	0.02*	(0.01)	0.03***	(0.01)	-0.02	(0.01)	-0.01	(0.01)
Vote share SPD 2013	-0.01	(0.01)	0.01	(0.01)	0.02	(0.01)	0.03**	(0.01)	-0.00	(0.02)	-0.00	(0.01)
Vote share Linke 2013	0.00	(0.01)	0.00	(0.01)	0.03*	(0.01)	0.03**	(0.01)	-0.05***	(0.01)	-0.01	(0.01)
Vote share AfD 2013	-0.02	(0.01)	0.01	(0.02)	0.05	(0.02)	0.06**	(0.02)	-0.03	(0.02)	-0.04	(0.02)
Brandenburg (ref.)												
Mecklenburg-Vorpommern	-0.10	(0.09)	0.05	(0.10)	0.05	(0.12)	-0.02	(0.10)	0.06	(0.16)	0.15	(0.10)
Sachsen	-0.00	(0.08)	0.12	(0.12)	0.07	(0.11)	-0.03	(0.09)	0.02	(0.15)	-0.03	(0.09)
Sachsen-Anhalt	-0.02	(0.10)	-0.01	(0.10)	0.14	(0.13)	0.11	(0.10)	-0.06	(0.16)	-0.08	(0.11)
Thüringen	-0.13	(0.09)	-0.02	(0.11)	0.08	(0.11)	0.01	(0.10)	0.05	(0.14)	-0.02	(0.10)
Constant	-1.77	(1.27)	0.26	(1.71)	-1.15	(1.78)	-1.47	(1.48)	0.57	(2.19)	2.49	(1.64)
Observations	1320		1297		1309		1320		1187		1320	
R ²	0.12		0.02		0.07		0.08		0.09		0.08	

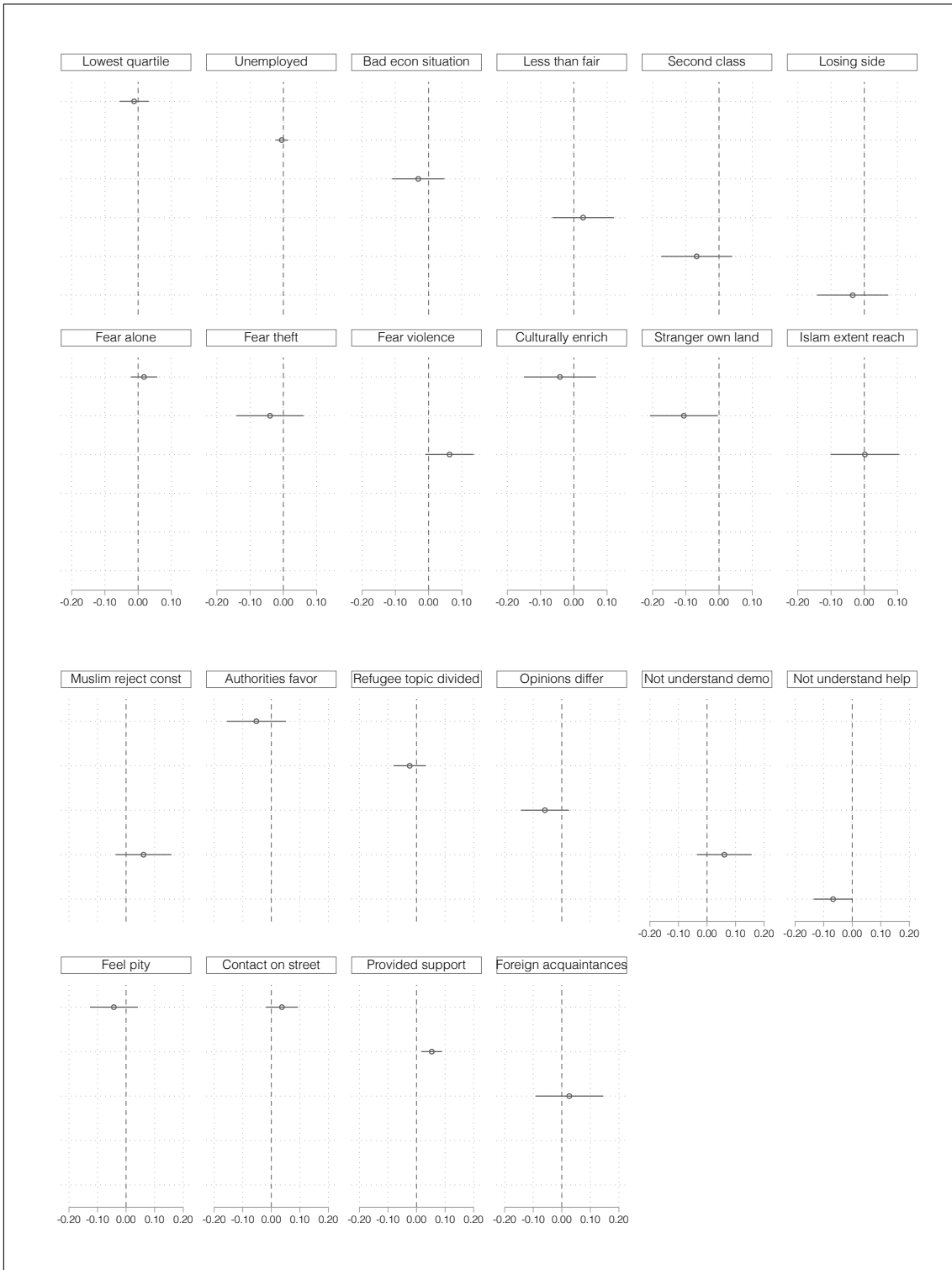
OLS regression. Difference to full sample size (n=1,320) due to missing items on individual outcome measures. Cluster robust standard errors in parentheses. SE clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 14A: Regression of individual outcome indicators on treatment indicator



Coefficient plot for regression of individual outcome indicators on treatment indicator, simultaneously controlling for full set of individual- and municipality-level pre-treatment covariates. Markers are point estimates, lines 95% confidence intervals. SE clustered at the municipality level.

Figure 15A: Regression of individual indicators for mechanisms on treatment indicator



Coefficient plot for regression of individual indicators for mechanisms on treatment indicator, simultaneously controlling for full set of individual- and municipality-level pre-treatment covariates. Markers are point estimates, lines 95% confidence intervals. SE clustered at the municipality level.

D.1 Heterogeneous treatment effects

Table 10A: Heterogeneity of treatment along left-right ideological dimension

	(1)		(2)		(3)		(4)	
	Right-wing support		Anti-refugee sentiments		Populism		Behavioral bias	
Treat#Left (ref.)								
Treat#Center	-0.02	(0.07)	-0.06	(0.09)	-0.10	(0.08)	0.01	(0.10)
Treat#Right	-0.11	(0.11)	-0.12	(0.13)	-0.13	(0.13)	-0.09	(0.16)
Left (ref.)								
Center (const.)	0.40***	(0.06)	0.45***	(0.08)	0.11	(0.06)	0.10	(0.08)
Right (const.)	1.08***	(0.09)	0.94***	(0.11)	0.47***	(0.11)	0.59***	(0.11)
Treatment municipalities=1	0.03	(0.06)	0.05	(0.08)	0.06	(0.06)	0.01	(0.08)
Constant	-0.88	(1.27)	-2.44	(1.50)	-0.11	(1.87)	1.07	(1.92)
Individual controls	Yes		Yes		Yes		Yes	
Municipality controls	Yes		Yes		Yes		Yes	
Observations	1,228		1,225		1,228		1,228	
R ²	0.34		0.25		0.08		0.09	

Cluster robust standard errors in parentheses. SE clustered at the municipality level. , * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

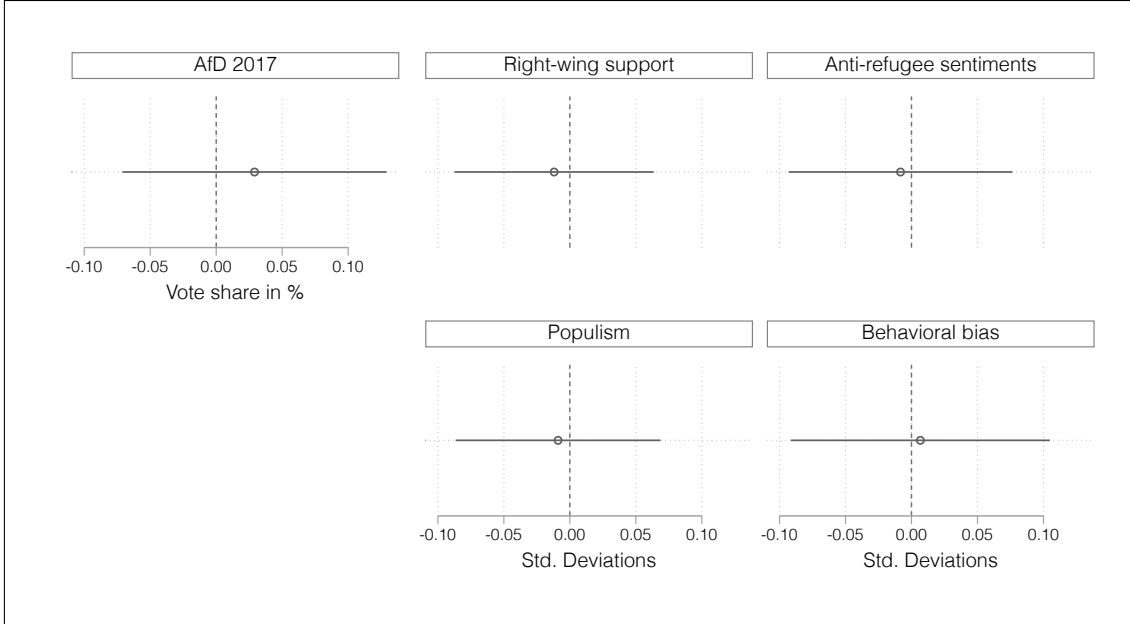
Table 11A: Heterogeneity of treatment along libertarian-authoritarian dimension

	(1)		(2)		(3)		(4)	
	Right-wing support		Anti-refugee sentiments		Populism		Behavioral bias	
Treat#Libertarian (ref.)								
Treat#Moderate	-0.12	(0.11)	-0.20	(0.11)	-0.16	(0.12)	-0.12	(0.16)
Treat#Authoritarian	-0.26	(0.13)	-0.33*	(0.13)	-0.23	(0.15)	-0.31	(0.20)
Libertarian (ref.)								
Moderate (const.)	0.45***	(0.09)	0.60***	(0.08)	0.40***	(0.10)	0.37**	(0.13)
Authoritarian (const.)	0.81***	(0.10)	0.98***	(0.10)	0.64***	(0.12)	0.61***	(0.17)
Treat (const.)	0.13	(0.10)	0.19	(0.10)	0.14	(0.12)	0.13	(0.15)
Constant	-1.90	(1.42)	-2.52	(1.47)	-0.43	(1.67)	0.06	(1.84)
Individual controls	Yes		Yes		Yes		Yes	
Municipality controls	Yes		Yes		Yes		Yes	
Observations	1,312		1,305		1,308		1,312	
R ²	0.09		0.12		0.05		0.02	

Cluster robust standard errors in parentheses. SE clustered at the municipality level. , * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D.2 Regression using age and gender weights

Figure 16A: Regression of outcomes on treatment, weighted to match age and gender margins in sampled municipalities



The Figure replicates the coefficient plot in Figure 5 in the main text. As can be seen, weighting to match the age and gender margins of the population in the sampled municipalities (shown in Table 19A) hardly effects the point estimates.

E Robustness checks

The following section presents robustness checks. We inquire whether results could be driven by a i) discontinuities along the continuum of different treatment dosages (intensive margins), ii) differences between individuals correctly and incorrectly classifying their municipality as having received refugees iii) spatial autocorrelation, i.e. spillover effects from treatment into control municipalities, iv) municipalities being ‘prevented’ from housing refugees by violence against refugees or refugee housing, and v) selective attrition.

E.1 Treatment dosage

Table 12A replicates the analysis of Table 7A using the percentage of refugees received relative to the population as independent variable. The results largely mirror those when using the binary indicator, showing null effects for all individual-level indicators. This said, there is a statistically

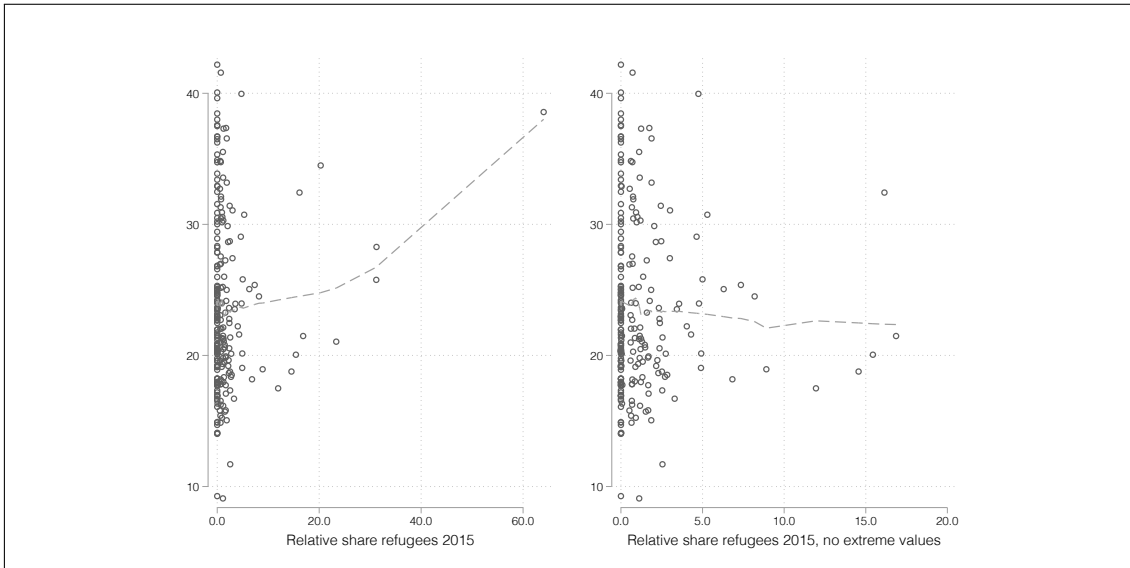
significant positive effect of the share of refugees relative to the resident population on the AfD vote share (Column 1). Further analyses (Figure 17A, Column 2) show that this effect relies on 5 municipalities that received extreme numbers of refugees, between 20% and 60% relative to their population. Excluding these municipalities, the relationship between right-wing party support and exposure vanishes.

Table 12A: Regression of outcome measures on number of refugees per 100 inhabitants of the resident population

	(1)		(2)		(3)		(4)		(5)		(6)	
	AfD 2017		AfD 2017, no outlier		Right-wing support		Anti-refugee sent.		Populism		Behavioral bias	
N refugees/100 inhabitants 2015	0.18***	(0.04)	0.11	(0.10)	0.00	(0.00)	0.00	(0.00)	-0.01	(0.00)	-0.00	(0.00)
Female					-0.12**	(0.04)	-0.10*	(0.04)	0.05	(0.04)	-0.13**	(0.05)
Age					-0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)
Single					0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)
Stable union					0.12	(0.07)	0.17*	(0.07)	0.10	(0.07)	0.00	(0.08)
Separated					0.16	(0.09)	0.16	(0.10)	0.07	(0.09)	-0.00	(0.11)
Widowed					0.10	(0.10)	0.15	(0.10)	0.19	(0.10)	-0.04	(0.13)
Children					-0.01	(0.02)	-0.06*	(0.03)	-0.04	(0.02)	0.00	(0.04)
Household size					0.00	(0.02)	0.02	(0.02)	-0.02	(0.02)	-0.00	(0.03)
Education					-0.14***	(0.02)	-0.14***	(0.02)	-0.09***	(0.02)	-0.09***	(0.02)
Area in km2	-0.01	(0.01)	-0.01	(0.01)	-0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)
Population	0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	0.00	(0.00)	-0.00	(0.00)	-0.00	(0.00)
Population density	-0.01	(0.01)	-0.01	(0.01)	0.00	(0.00)	-0.00	(0.00)	-0.00*	(0.00)	-0.00	(0.00)
Unemployment	0.32	(0.20)	0.26	(0.20)	0.01	(0.02)	-0.01	(0.02)	0.03	(0.02)	0.01	(0.02)
Share female	0.08	(0.21)	0.08	(0.20)	-0.01	(0.02)	-0.01	(0.02)	-0.01	(0.02)	-0.02	(0.02)
Average age	-0.17	(0.26)	-0.08	(0.27)	0.03	(0.03)	0.06*	(0.03)	-0.00	(0.03)	-0.01	(0.03)
Share over 65 years	0.06	(0.12)	0.05	(0.12)	-0.01	(0.02)	-0.02	(0.01)	0.01	(0.01)	0.01	(0.01)
Share men 15-25 years	0.96*	(0.37)	1.02**	(0.37)	-0.05	(0.04)	-0.04	(0.04)	-0.04	(0.04)	-0.09	(0.07)
Share foreigners	-0.99	(0.90)	-0.89	(0.91)	-0.09	(0.08)	0.02	(0.10)	0.16	(0.09)	-0.16	(0.10)
Purchasing power	-0.24	(0.17)	-0.19	(0.17)	-0.02	(0.01)	-0.01	(0.02)	0.01	(0.02)	0.02	(0.02)
Turnout 2013	-0.15**	(0.05)	-0.13**	(0.05)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.01	(0.01)
Vote share CDU 2013	-0.35***	(0.10)	-0.34***	(0.10)	0.02*	(0.01)	0.01	(0.01)	-0.00	(0.01)	0.01	(0.01)
Vote share SPD 2013	-0.68***	(0.11)	-0.74***	(0.12)	0.01	(0.01)	0.00	(0.01)	0.01	(0.01)	0.02	(0.01)
Vote share Linke 2013	-0.16	(0.12)	-0.14	(0.12)	0.02*	(0.01)	0.02	(0.01)	0.01	(0.01)	0.02	(0.01)
Vote share AfD 2013	0.87***	(0.18)	0.91***	(0.18)	0.05**	(0.02)	0.02	(0.02)	-0.00	(0.02)	0.02	(0.02)
Brandenburg	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)
Mecklenburg-Vorpommern	-4.70***	(1.03)	-4.66***	(1.03)	-0.07	(0.09)	-0.10	(0.11)	0.24*	(0.11)	-0.07	(0.12)
Sachsen	7.61***	(1.21)	6.93***	(1.22)	0.01	(0.09)	-0.10	(0.10)	0.30***	(0.09)	-0.01	(0.13)
Sachsen-Anhalt	-2.09	(1.16)	-2.27*	(1.14)	0.07	(0.10)	-0.14	(0.12)	0.14	(0.12)	0.00	(0.12)
Thüringen	0.08	(1.16)	-0.36	(1.17)	0.06	(0.09)	-0.08	(0.10)	0.23*	(0.09)	-0.05	(0.10)
Constant	60.46***	(15.70)	54.81***	(15.90)	-1.70	(1.46)	-2.33	(1.56)	0.07	(1.71)	0.31	(1.89)
Observations	236		231		1,319		1,311		1,314		1,320	
R ²	0.71		0.72		0.09		0.08		0.05		0.03	

OLS regression. Difference to full sample size (n=1,320) due to missing items on individual outcome measures. Cluster robust standard errors in parentheses. SE clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 17A: Locally weighted regression of the vote share garnered by the AfD in the study area during the 2017 general elections on the percentage of refugees relative to the population.

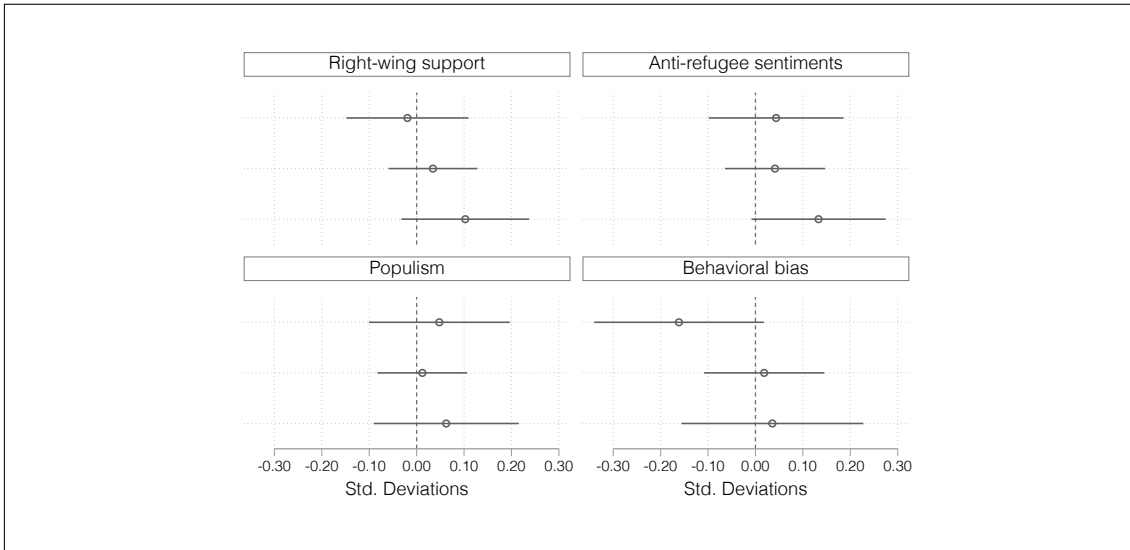


Positive relationship due to five observations with very high numbers of refugees relative to the resident population (>200 per 1,000 inhabitants). On the left: including outliers, corresponding to Column 5 in Table 12A. On the right: excluding outliers, corresponding to Column 6 in Table 12A.

E.2 Perceived vs. actual treatment

As could be seen in Table 1, in our sample we observe quite high shares of individuals classifying their municipality as having received refugees although, in fact, this was not the case. Likewise, there are individuals classifying their municipality as not having received refugees even though this was actually the case. To check whether this phenomenon affects our results, we split our sample in four groups: i) those who live in municipalities that host refugees and are aware of this fact (correct positives), ii) those living in host communities but are unaware of this (false negatives), iii) those living in non-host communities and aware of this (correct negatives), and iv) those living in non-host communities, but believing they live in host communities (false positives). We then estimate treatment effects relative to the correct negatives for the three other groups. The results are depicted in Figure 18A. We see that ‘false positives’—those who erroneously believe that their municipality received refugees—tend to exhibit a slightly larger treatment effects than even the ‘correct positives’. In no cases are effects statistically distinguishable from zero, however.

Figure 18A: Treatment effects for individuals correctly and incorrectly classifying their municipality as having received refugees



Treatment effects relative to those correctly classifying their municipality as not having received refugees for individuals who a) correctly classify their municipality as having received refugees (correct positives), b) incorrectly classify their municipality as *not* having received refugees (false negatives), and c) incorrectly classify their municipality as having received refugees even though this was not the case (false positives). Markers are point estimates, horizontal lines 95% confidence intervals. Standard errors clustered at the municipality level.

E.3 Spatial spillover

In addition, we may worry about spatial spillovers or spatial autocorrelation. Could it be that the treatment—receiving refugees—affected treatment and control municipalities alike? Or that receiving refugees at least had a strong influence on adjacent control municipalities? As outlined, we attempted to shield against such concerns by implementing a minimal distance threshold between treatment and control municipalities during the matching process. What is more, in the section below, we demonstrate that respondents in the treatment municipalities were clearly aware of the presence of refugees in their close vicinity, while those in control municipalities were not. It therefore seems unlikely that the treatment simply had the same effect in both the treatment and control condition. Nevertheless, it is worthwhile to try to quantify the degree of spatial spillovers/ autocorrelation present in our data.

This is done in Tables 13A and 14A, which present the results of two tests. These analyses are conducted with a municipality-level dataset, consisting of averages of our dependent and independent variables. Due to missing values, one observation has to be dropped so that we are

Table 13A: Moran's I measure of spatial dependency

	Moran's I	p	N
Behavioral bias	-0.011	0.19	235
Right-wing support	-0.007	0.36	235
Populism	-0.007	0.35	235
Reject refugees	-0.008	0.34	235
AfD vote share 2017	0.200	0.00	235
I_0	-0.004		

working with 235 observations. Both tests rely on a spatial weight matrix \mathbf{W} , which we specify as the pairwise crow-fly distances between municipality centroids. As a first test, we checked for the presence of global spatial dependency in our data by calculating Moran's I . The expected value for I in the absence of spatial autocorrelation is $I_0 = -1/(n - 1)$, i.e. $-1/(235-1)=-0.0043$. As can be seen none of the estimated I s for the four individual-level outcome measures deviates significantly from that value, indicating an overall lack of spatial dependency for these measures. For the election outcomes, in contrast, Moran's I detects the presence of spatial autocorrelation. Neighboring municipalities show similar voting patterns, perhaps as a result of regionally (rather than locally) targeted campaigns.

Table 14A: Spatial regression of outcome measures on treatment status and spatial lag

	(1)		(2)		(3)		(4)		(5)	
	AfD 2017		Right-wing support		Anti-refugee sentiments		Populism		Behavioral bias	
Treatment municipalities	0.23	(0.46)	0.05	(0.05)	0.04	(0.06)	-0.01	(0.06)	-0.07	(0.07)
Individual controls	Yes		Yes		Yes		Yes		Yes	
Municipality controls	Yes		Yes		Yes		Yes		Yes	
ρ	-0.83***	(0.14)	-0.72	(0.54)	-0.61	(0.51)	-0.68	(0.51)	-1.16	(0.60)
Observations	235		235		235		235		235	

Spatial regression of indicated outcomes on treatment indicator, controls and spatial lag ρ . Cluster robust standard errors in parentheses. SE clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These results are confirmed with a second test, for which we estimate regression models with a spatially lagged dependent variable (Anselin, 1988). These models have the outcome scales of a given municipality on the left-hand side of the equation, just as in a normal OLS model. However, in addition, the outcome values for all other interview locations are added onto the right-hand side of the equation, weighted with weights ω from the matrix \mathbf{W} . The full model can be written as $y = \mathbf{X}\beta + \rho\mathbf{W}y + \epsilon$, where y is the given outcome, the matrix \mathbf{X} includes the treatment indicator and controls, and ϵ is an error term. Spatial dependency in the data is captured by the coefficient for the weighted spatial lag, ρ . The point estimates and significant levels for ρ are shown in Table 14A.

Again, we see no evidence for spatial autocorrelation for the individual-level outcome measures, and the estimates for the treatment effect consequently are hardly moved. For the AfD vote share during the 2017 elections, ρ is clearly positive, indicating a degree of spatial dependence in the data. Yet again, the point estimate for the treatment effect remains indistinguishable from zero.

E.4 ‘Prevented’ refugee allocation due to violence against refugees or refugee housing

Further, we may be concerned that certain municipalities did not receive refugees because they saw resistance in the population in the form of protests or attacks against designated housing (Krueger & Pischke, 1997). If this was the case, our null-results would be less remarkable—at least if we expected an overall negative effects of housing refugees: municipalities with pre-existing anti-immigrant sentiments would receive no immigrants, whereas attitudes towards them would sour in receiving municipalities. Given our matching design, this is an unlikely scenario. As we discussed, treatment and control municipalities were indistinguishable in terms of pre-treatment voting outcomes during the 2013 general elections. Strong pre-existing differences in anti-refugee sentiments would likely have reflected on these election results. To nevertheless address this worry, we run an additional test. We use a dataset on attacks on refugees and refugee housing provided by Benček and Strasheim (2016). The dataset comprises 1,645 records of attacks carried out during 2014 and 2015.

In order to avoid post-treatment bias, we restrict ourselves to the 1,201 events that occurred before September 2015, i.e. before the onset of the refugee crisis proper. The dataset allows us to distinguish between two types of events: attacks against persons, such as bodily assault, and attacks against buildings, such as arson or other forms of vandalism (like opening the water tap to flood a building and make it uninhabitable). Overall, we would expect our municipalities in focus to see low numbers of attacks since there were few refugees around. This said, if the suspicion is true that control municipalities were more hostile to start with, we should see a higher number of attacks here. If, in contrast, attacks are driven largely by opportunity, we might see more attacks in treatment municipalities, especially against designated refugee housing. After all, the availability of housing is what marked them out as suitable for hosting refugees.

A comparison of anti-refugee events is provided in Table 15A. Overall, there were five demonstrations, two acts of arson, and ten acts of other forms of vandalism against refugee housing.

Table 15A: Attacks and demonstrations against refugees and refugee housing

	Overall		Control		Treatment	
	Mean	SD	Mean	SD	Mean	SD
Anti-refugee demonstration	0.02	(0.27)	0.03	(0.37)	0.01	(0.09)
Arson against refugee housing	0.01	(0.09)	0.01	(0.09)	0.01	(0.09)
Vandalism against refugee housing [†]	0.05	(0.23)	0.02	(0.13)	0.08	(0.30)
Bodily assault of refugee	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)

Differences between treatment and control condition significant at $p < 0.05$ marked with a dagger (†) symbol. Figures for attacks/demonstrations occurring before September 2015 from Benček and Strasheim (2016).

No refugee was bodily assaulted. Of the demonstrations, four took place in one single control municipality, and one in a treatment municipality. Arson attacks took place in one treatment and one control municipality each. The only difference that reaches statistical significance are other acts of vandalism against refugee housing, two of which took place in control municipalities, and eight in treatment municipalities. The evidence therefore refutes the idea that pre-existing levels of hostility could have driven the allocation of refugees, save for in the case of the control municipalities that saw four demonstrations. On the contrary, it appears that acts of violence are largely driven by opportunity. The absence of refugees prevented attacks on them, and greater abundance of potential refugee housing in the treatment municipalities made arson and other forms of vandalism more likely.

E.5 Attrition

We may also worry that our results are driven by selective attrition—people selectively moving out of, or into, our municipalities in focus. If those most opposed to welcoming newcomers moved out of treatment municipalities, this would tend to bias negative effects downwards. And if the same people moved to control municipalities, taking their negative attitudes with them, this argument would apply even stronger. Worried about such effects, we collected data on residential mobility. Specifically, we asked our respondents how many individuals have moved in or out of their neighborhood during the last three years, and how long they themselves have been living in their current location. The answers to these questions are summed up Table 16A above. We see that there are no differences in the number of people who have moved out between the control and treatment condition. A two-sided t-test results in a p-value of 0.71. Furthermore, in terms of people who have moved in, if anything, for participants in treatment municipalities we find slightly higher

values—which could be expected given that these municipalities received refugees—although again the two conditions are statistically indistinguishable ($p=0.31$). This makes it very unlikely that the selective attrition story just outlined holds.

Table 16A: Attrition checks

	Overall		Control		Treatment	
	Mean	SD	Mean	SD	Mean	SD
<u>Attrition</u>						
N neighbors moved into neighborhood	1.68	(0.64)	1.66	(0.61)	1.69	(0.66)
N neighbors moved out of neighborhood	1.43	(0.61)	1.44	(0.62)	1.42	(0.61)
Local resident for 4+ years	0.93	(0.26)	0.92	(0.28)	0.93	(0.25)
N	1,320		512		808	

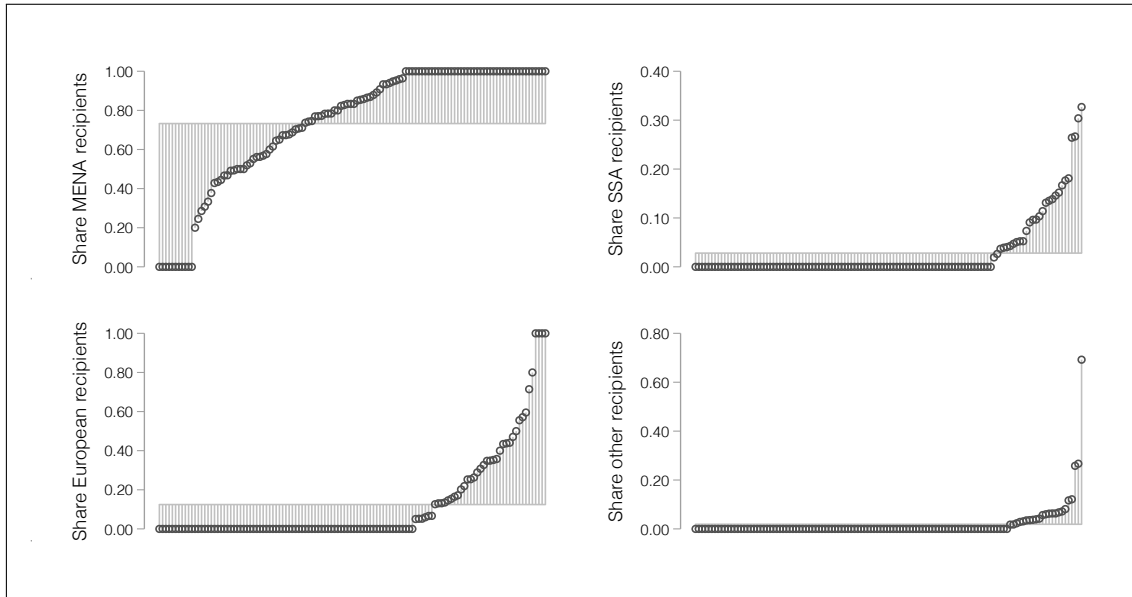
Differences between treatment and control condition significant at $p<0.05$ marked with a dagger (†) symbol.

Arguably even more important, we look at regions with very high levels of residential stability. 93% of participants indicated that they had been living in the same house or flat for at least 4 years, before the onset of the refugee crisis. When restricting our sample to these long-term residents only (not shown), results closely replicate those presented in Table 7A in the main text.

E.6 Origin effects

Finally, we may wonder whether our results could be influenced by the origin of asylum seekers allocated to the communities. Residents might react differently to asylum seekers from different places of origin. While this should not change our overall/average null result, it is interesting to explore whether groups of certain origin trigger positive reactions, whereas others elicit negative responses. For data protection reasons, our information on the origin of the asylum seekers is quite coarse. We could only obtain the share of asylum seekers allocated to a given municipality for four rough groups: asylum seekers from 1) Europe (12% of all asylum seekers in the treatment municipalities), 2) the MENA (Middle East and Northern Africa) region (73%), 3) from Sub-Saharan Africa (3%), and from 4) ‘other’ regions (2%, deviation from 100% due to unknown origins). As a first step, we can ask whether these groups were allocated randomly to the municipalities, which would imply that the distribution over all municipalities should be approximately uniform. Figure 19A shows that this was not the case. Instead, certain municipalities were allocated disproportionately many individuals from a certain origin but not another.

Figure 19A: Deviation from mean for shares of asylum seekers of indicated origin allocated to a given municipality



All deviations from the uniform distribution significant at $p < 0.001$ in Kolmogorov-Smirnov tests.

But do these differences in the place of origin matter for our outcomes? In order to assess this question, we run a regression model where, instead of the treatment indicator, we add the place-of-origin shares for the four groups. The results of this exercise are presented in Table 17A below. As can be seen, there seem to be slight differences in the ways asylum seekers from different locations influence the outcomes—but all of these differences stay well clear of conventional levels of statistical significance. The non-random distribution of asylum seekers does not seem to have influenced our outcomes in any meaningful way.

Table 17A: Regression of outcomes on share of asylum seekers from different places of origin

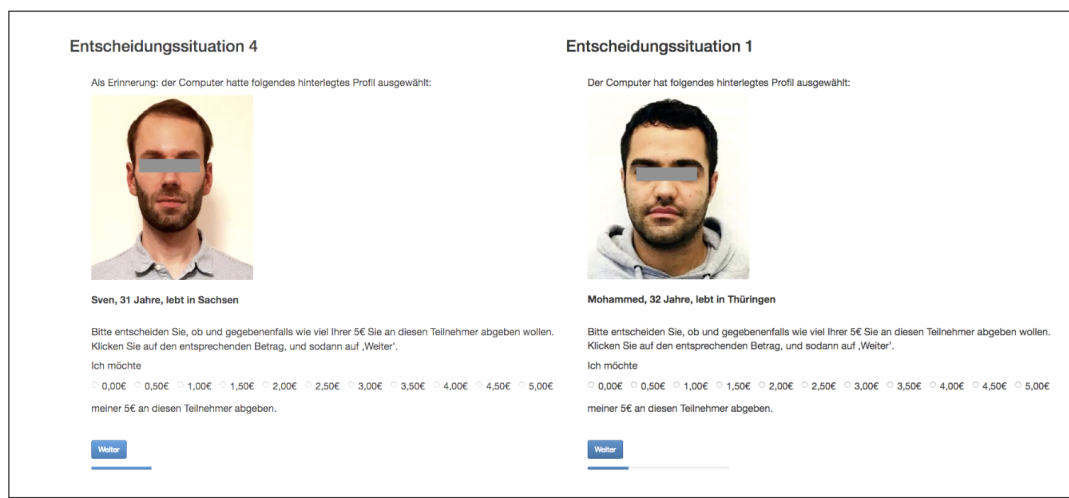
	(1)		(2)		(3)		(4)		(5)	
	AfD 2017		Right-wing support		Anti-refugee sentiments		Populism		Behavioral bias	
Share MENA recipients	0.33	(0.62)	0.03	(0.08)	0.00	(0.05)	-0.05	(0.05)	0.01	(0.06)
Share SSA recipients	-2.71	(5.26)	-0.28	(0.31)	-0.02	(0.40)	0.20	(0.32)	-0.01	(0.43)
Share European recipients	-0.54	(1.42)	-0.10	(0.13)	-0.13	(0.11)	-0.00	(0.11)	-0.09	(0.14)
Share other recipients	-1.29	(4.89)	-0.28	(0.27)	0.22	(0.29)	0.14	(0.24)	0.07	(0.35)
Constant	61.37***	(16.92)	-1.70	(1.45)	-2.40	(1.63)	-0.15	(1.77)	0.06	(1.92)
Individual controls	Yes		Yes		Yes		Yes		Yes	
Municipality controls	Yes		Yes		Yes		Yes		Yes	
Observations	236		1319		1311		1314		1320	
R^2	0.70		0.09		0.08		0.05		0.04	

Spatial regression of indicated outcomes on treatment indicator, controls and spatial lag ρ . Cluster robust standard errors in parentheses. SE clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

F Behavioral games

The interaction partners in the behavioral games were represented by means of pictures, including their typical German or immigrant-sounding first name, information about their age and the state of residency. Examples of the Dictator Game with in- and out-group interaction partners are shown in Figure 20A.

Figure 20A: Dictator game for the online experiment



In applying this technique, we follow other scholars such as Habyarimana, Humphreys, Posner, and Weinstein (2009). Ahead of fielding the main survey, we recruited a first set of 37 participants who were willing to share their photo with us and participate in a pre-test of the behavioral games. We recruited both prototypically German-looking individuals to represent the ingroup, and individuals with a visible immigrant-background (Middle Eastern phenotype) to represent the outgroup. In order to check that the difference would be recognizable, and to minimize the influence of factors other than the ingroup/outgroup distinction on the decision of our participants, we ran extensive pre-tests on our photos. In particular, we had workers on Amazon’s Mechanical Turk platform rate the photos in terms of perceived expressed emotions (happy, angry, fearful, sad, neutral) and attractiveness—factors known to influence facial cues for trustworthiness and cooperative decisions Andreoni and Petrie, 2008; Todorov, Olivola, Dotsch, and Mende-Siedlecki, 2015.

We then selected eight individuals (two prototypical German- and two immigrant-looking females, two German- and two immigrant-looking males) that were closely matched across all perceived traits. All our participants in the main survey interacted with these eight individuals. For

Figure 21A: Graphical explanation of the trust game for the online experiment



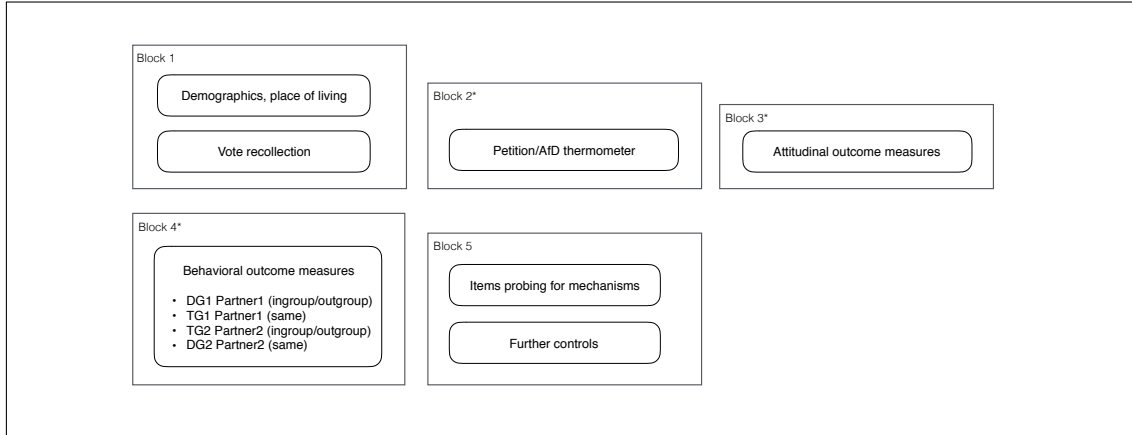
the strategic interactions in the trust game, we asked the first set of pre-test participants to solve the trust game using the strategy method. That is, for each amount received, they indicated how much money they would send back. The participants in the main survey were paid the corresponding amounts, and the pre-test participants whose pictures we used were paid the average amounts handed to them by the participants in the main survey. Figure 21A illustrates how the trust game was introduced.

Order of presentation of measures

Figure 22A illustrates the order in which the different measures and survey questions were presented. The study always started with a few questions on basic demographics like age and marital status. The three blocks that followed were presented in a randomized order so to avoid answers to questions influencing each other in a systematic way. These blocks covered i) respondents' recalled voting behavior, ii) the four game decisions in the dictator and trust games, and iii) items capturing

anti-immigrant and anti-refugee sentiments, and populist attitudes. Items probing for mechanisms were asked last.

Figure 22A: Order of measures



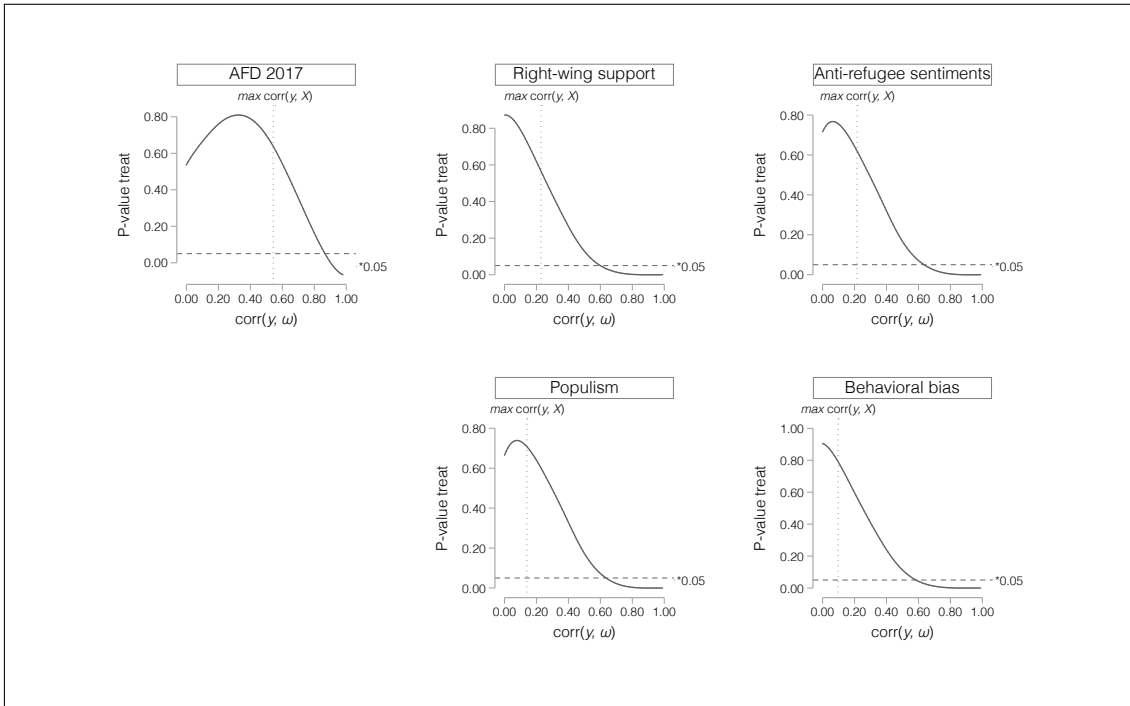
Order in which measures were presented. Blocks marked with an asterisk were presented in random order.

G Simulation of selection on unobservables

In order to further check the robustness of our null effects, we conduct a simulation exercise. We ask how strong the influence of an unobserved confounder ω would need to be to render the effect of the treatment statistically significantly different from zero. Confounding is due to two correlations: the correlation between a potential unobserved confounder ω and the outcome y ($corr(\omega, y)$), and the correlation between ω and the treatment indicator ($corr(\omega, treat)$). We generate ω as a random variable with a standard-normal distribution, and fix the correlation $corr(\omega, treat)$ at the level of the correlation between the treatment and empty housing stock, the strongest and only statistically significant predictor of treatment assignment. We then add ω as an additional predictor to our standard regression models with full controls. We increment the Pearson correlation between y and ω in steps of 0.01, starting at 0 and ending at 1.

At each step, we re-estimate our model and record the p-value for the treatment indicator. As a benchmark, the figures also include the value of the strongest correlation between a given outcome and any of the control variables included in our models. In the case of the AfD vote share 2017, this is the AfD vote share in 2013, and in the case of the individual-level outcomes, this is education. Figure 1 plots the results of the simulation exercise.

Figure 23A: Simulation of unobservable variable bias necessary to move treatment effect away from zero



We see that in all cases the correlation between the unobserved variable ω and the outcome would need to be very strong indeed to render the treatment effect statistically different from zero at the $p=0.05$ level—at least around 0.6, and up to 0.85. How unlikely it is to observe such correlations becomes clear when comparing these values to those for the strongest predictors: in all cases, the correlation $corr(\omega, y)$ would need to be at least two to three times stronger than that of the outcome with the strongest predictor $max\ corr(y, X)$. Since this seems exceedingly unlikely, we consider the simulation exercise to provide strong evidence for the robustness of the null effects we are observing.

H Sampling and recruitment procedures

Our empirical strategy relies on comparing attitudes and behavior in highly similar villages and small towns across Eastern Germany, some of which received asylum seekers—the treatment municipalities, and some of which that did not—the control municipalities. In defining our sample, we aimed at achieving maximum possible internal validity for our causal claim. The aim of our sampling and matching procedure therefore was to generate a sample of treatment and control municipalities indistinguishable along observable pre-treatment characteristics. The strategy was

spelled out before the start of data collection in our pre-analysis plan and registered online. A power analysis conducted in our pre-analysis plan showed that our sample would allow us to detect differences in outcomes in the the size of 0.16 standard deviations, with a power of 0.8 and assuming a type I error rate of 5%.

Our starting population were all municipalities in the five Eastern Germany (n=2,673 as of 31 Dec 2014). Since we were interested in first-time exposure, we limited our focus to those 83% of municipalities where before 2015, foreigners constituted a negligible share of the population (under 1.5%), and which did not host refugees (for a visual overview, see Figure 3a in the main text). In order to limit spillover effects, we further narrowed our focus to municipalities located at some distance (over 6.6 km) from municipalities excluded in the first step. This left us with a sample of 729 municipalities. We defined municipalities as treated if they received a significant number of refugees (a number of individuals corresponding to more than 0.4% relative to the population) for the first time in 2015/16, and as potential control if they did not. The exact criteria used in this selection procedure are the following:

A) Criteria that had to be met by all municipalities for being considered for our sample:

1. No previous hosting of refugees: Number of refugees hosted relative to population in 2014 <0.1%
2. Little previous exposure to regular immigrants: Share of foreigners in population <1.5% (figures from the 2011 census), the median value in our sample.
3. Sufficient distance to excluded municipalities: Distance to central point of excluded municipalities >6.6km, the median distance between treated and excluded municipalities.

B) Additional criteria for control units:

4. No subsequent hosting of refugees: Number of refugees hosted relative to population in 2015 <0.1%.
5. Sufficient distance from treatment units: Distance to the central point of treated municipalities >8.6km (again, the median distance).

C) Additional criterion for treatment units:

6. Significant treatment: Number of refugees hosted relative to population in 2015 >0.4%, the 25% percentile.

We then used a combination of exact and propensity-score (PS) matching to obtain our final sample. Before applying the PS-procedure, we exactly matched our municipalities in terms of the federal state (*Bundesland*) they belong to, and in terms of rough population size.

Table 18A: Relative population size and corresponding sample sizes

	Population		Sample (individuals)			Municipalities		
	Pop 2015	%	Total	Treat	Control	Total	Treat	Control
Brandenburg	2,484,826	20	282	181	101	62	31	31
Mecklenburg-Vorpommern	1,612,362	13	180	116	64	72	36	36
Sachsen	4,084,851	32	454	294	160	40	20	20
Sachsen-Anhalt	2,245,470	18	248	162	86	34	17	17
Thüringen	2,170,714	17	244	158	86	32	16	16
Sum	12,598,223	100	1408	911	497	240	120	120

States in Germany are polities in their own right, and refugee policies are partially made on this level. Therefore, legal procedures or political discourses at this level might shape how the reception of asylum seekers affects local attitudes. Exact matching on the *Bundesland* eliminates these concerns. We further stratify the matching process by population size to avoid matching very large municipalities with small ones.

We then moved on to apply propensity score matching within these 10 strata using the `psmatch2` package (Leuven & Sianesi, 2003). The propensity score (the probability of a municipality belonging to the treated group) was estimated by means of a probit regression of treatment assignment on a number of pre-treatment municipality covariates, including population size, population density, area size, purchasing power, unemployment rate, average age, shares of population for different age classes (over 65, under 18, between 15 and 25), male/female ratio and vote shares for the four largest parties during the last (2013) general elections. Treatment municipalities were then matched 1:1 to their nearest neighbor (the municipality with the most similar propensity score) from the control group. To prevent matches between nearest neighbors that nevertheless differ strongly in their propensity score, a caliper of 0.1 was used. This procedure left us with between 16 and 31 pairs of treatment and control municipalities in each of the federal states, for a total of 240 municipalities (Table 18A).

Table 18A shows the relative sample sizes per *Bundesland* that we aimed to recruit. That is, we sought to recruit 32% of our respondents in the state of Sachsen, 20% in Brandenburg, 18% in Sachsen-Anhalt, 17% in Thüringen, and 13% in Mecklenburg-Vorpommern. Within each municipality in a given *Bundesland*, we aimed to recruit participants proportional to the

Table 19A: Comparison demographics sample/population

Age	Sample			Population		
	Male	Female	Total	Male	Female	Total
18-29	3	3	6	6	5	11
30-59	26	34	60	27	25	52
60+	18	16	34	17	21	38
Total	47	53	100	50	50	100

Sample: Cell frequencies in percent for study participants. Population: Frequencies from population statistics for 236 treatment and control municipalities, 2015 figures (Statistische Ämter des Bundes und der Länder, 2016).

relative population size compared to the full sample. For example, in a treatment municipality in Sachsen that hosts 3% of the total inhabitants in the treatment municipalities, we sought to recruit $0.03 \cdot 195 \approx 6$ participants, and from one that hosts 8%, $0.08 \cdot 195 \approx 16$ participants. As demonstrated in Table 1 in the main text and Table 20A, the matching procedure resulted in excellent balance across the treatment and control conditions.

Table 20A: Regression of treatment on covariates

	Municipality hosting refugees		
	b	se	ci95
Area in km2	0.00	0.00	-0.00,0.00
Population 2014	-0.00	0.00	-0.00,0.00
Population density 2014	0.00	0.00	-0.00,0.00
Unemployment rate 2014	0.07	2.72	-5.28,5.43
Share women in 2011	-0.03	0.03	-0.08,0.03
Average age in 2011	0.01	0.04	-0.06,0.08
Share of population over 65 in 2011	0.00	0.02	-0.03,0.04
Share of men 15 to 25 years in 2011	-1.02	5.15	-11.18,9.13
Share foreigners	-0.01	0.12	-0.25,0.23
Purchasing power pp 2005	-0.00	0.00	-0.00,0.00
Voter turnout in %	0.00	0.01	-0.01,0.02
Share CDU votes in %	-0.02	0.02	-0.06,0.03
Share SPD votes in %	-0.02	0.02	-0.07,0.03
Share LINKE votes in %	-0.02	0.02	-0.06,0.03
Share AfD votes in %	-0.04	0.06	-0.16,0.09
Brandenburg (ref.)			
Mecklenburg-Vorpommern	0.07	0.14	-0.21,0.34
Sachsen	-0.04	0.17	-0.37,0.30
Sachsen-Anhalt	0.05	0.16	-0.26,0.36
Thüringen	-0.00	0.16	-0.32,0.31
<i>Share empty housing[†]</i>	0.08	0.03	0.02,0.15
<i>Mayor independent (ref.)</i>			
<i>Mayor CDU</i>	0.05	0.14	-0.22,0.32
<i>Mayor Linke</i>	-0.12	0.24	-0.60,0.36
<i>Mayor SPD</i>	0.12	0.20	-0.28,0.52
<i>Mayor from ruling party</i>	-0.10	0.16	-0.42,0.21
Constant	2.79	2.92	-2.97,8.54
<i>N</i>	240		
<i>F(24, 215)</i>	0.46		
<i>R²</i>	0.049		

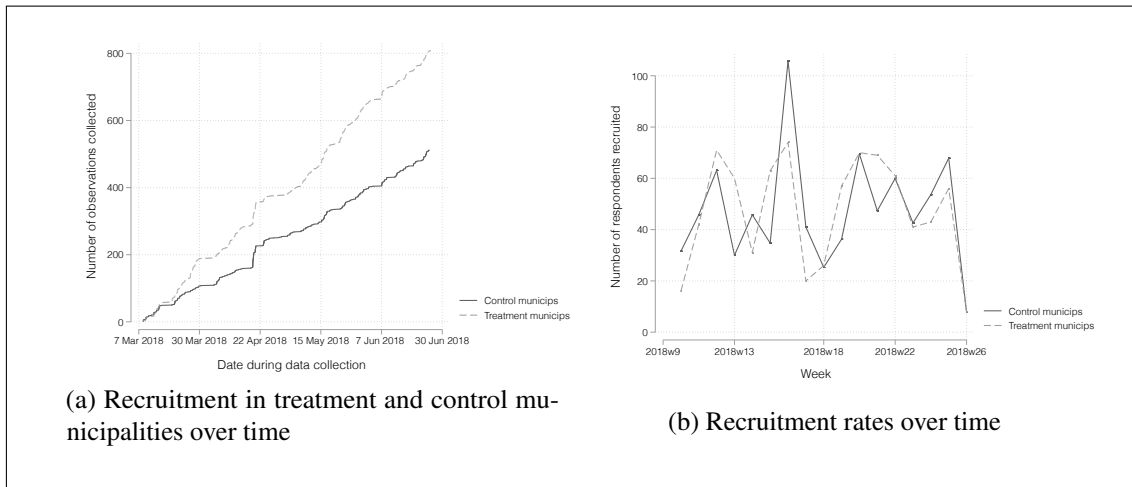
Regression of treatment indicator on matching variables, using matched sample. DV: Municipality received refugees in 2015. Text in italics indicates variables collected post-matching/not in pre-registration plan. Coefficients significant at the 5%-level marked with a dagger symbol (†).

H.1 Recruitment dynamics over time

Recruitment took place between March and June 2018, and was conducted by the CATI research lab of the University of Jena. Municipalities were called in random order, recruiters and enumerators were unaware of the treatment status of the municipalities, and respondents from treatment and control municipalities were recruited simultaneously. These measures served to ensure that no bias would be introduced by the differential recruitment of participants with different treatment statuses. Figure 24Aa demonstrates that respondents filled in the survey equal at rates in both treatment and control municipalities.

A worry may be that individuals in treatment municipalities were less likely to participate in the study than those in control municipalities, and therefore bias our sample. Figure 24A demonstrates

Figure 24A: Recruitment



Note: Figure 24A shows the number of observations collected each calendar week between March and June 2018 in treatment and control municipalities. To allow for better comparability, the recruitment rate in control municipalities has been scaled up by a factor of $808/512=1.58$, reflecting the relatively larger number of observations collected in treatment as compared to control municipalities.

that this concern is not supported by our data. Instead, the figure shows that recruitment rates resembled each other, suggesting that the willingness to participate did not systematically vary across treatment conditions.

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